

Operationism Still Isn't Real: A Temporary Reply To Kendler

Thomas H. Leahey

Virginia Commonwealth University

Kendler's defense of operationism is briefly rebutted, pending a fuller reply. After a few minor disagreements are dispensed with, problems with Kendler's account are raised. It is argued that Kendler's own examples of operational definition either demonstrate that when useful, they aren't operational, or when operational, they aren't useful. Nor does my critique depend on Kuhnian repudiation of "immaculate perception." Most importantly, however, Kendler's attempt to detach operationism from its philosophical context merely smuggles that context into psychology unexamined.

I am, of course, grateful to Ray Russ for the opportunity to reply to Kendler's defense of operationism against my earlier attack in *JMB* (Leahey, 1980). But, of course, I was distressed to learn I had at most two weeks to do so! So we agreed that I would provide an immediate, very brief reply for publication along with Kendler's paper, to be followed by a fuller reply to appear—I hope—in the issue after this one. In the present paper I will dispense with quibbles between Kendler and myself, and then raise more serious issues to be fully explored later. One major problem with Kendler's rejoinder is that it is derived largely from his recent book (Kendler, 1981), and it consequently misfires against my analysis of operationism, which is only partly based on the views Kendler considers in his book and article. Specifically, Kendler says nothing about my formulations of operational practice or my examples of operational definition from the psychological literature. Therefore, in my lengthier paper I will discuss the broader framework of operationism that Kendler builds in his book.

Quibbles

Minor issues make for poor prose, and I will dispense with a few of them in the order they occur in Kendler's paper.

Kendler considers that I share with other critics of operational definition the mistaken belief that it is "the sole form of scientific definition."

Now as Kendler develops his own account of scientific meaning this is true, since he accepts three other kinds of definition. But the original proposal of the operationists was in fact to accept operational definitions as the only scientifically valid ones. By eliminating supposedly subjective and scientifically worthless "surplus meaning" operationism would—it was hoped—ensure scientific rigor and verifiability. Kendler himself welcomes surplus meaning back into scientific psychology as "intuitive" meaning, and hints that earlier psychologists may have gone overboard in their zealous pursuit of operational precision, but this does not at all defuse my claim—indeed it supports it—that operationism has had baneful effects on psychological theory and research. Precisely because operationism is overly restrictive, Kendler has had to greatly expand what may be accepted as scientifically meaningful.

Kendler disputes my claim that operationism exerts a "powerful hold" on psychologists. Ultimately, this issue would have to be resolved by opinion poll, but my impression from reading in current psychology is that operational rhetoric is frequently used today, especially when we are asserting that one's theory is scientific or when challenging someone else's theory as less than rigorous. It certainly is true that operationism is no longer openly debated—something I hope I have remedied with my article—but only because it has become a familiar background concept for psychologists. That is why I borrowed Don Dulany's image of operational usage as empty liturgy. Its comfortable phrases reassure us that we are being scientific even though we no longer try to canonically formulate operational definitions. In large measure I would be satisfied if psychologists would just keep doing what they are doing—pursuing testable theories—and drop the unnecessary operational mumbo-jumbo. Elephants will not be found in New York whether or not the patient keeps clapping; psychology's scientific status will not change, despite our fears, whether or not we say we are "operationalizing."

A similar opinion-poll issue concerns whether or not philosophers of science generally agree that logical positivism is inadequate. My own continued reading in philosophy of science leads me to conclude that such agreement does exist, and a widely respected historical account of philosophy of science by a leading philosopher (Suppe, 1974) also concludes that logical positivism is effectively dead. But of course, the size of one's argumentative army is not supposed to settle the sort of dispute between Kendler and myself; rather, quality of argument is.

The force of Kendler's citation of Ghiselin's (1969) conclusion that Darwin was a positivist I find unclear. In the first place, that one scientist worked this way does not mean that everyone else *ought* to work the same way, no matter how great his success. More fundamentally, Ghiselin's conclusion may be disputed (e.g., Hull, 1978).

Of equally unclear force is Kendler's appeal to the history of science ("e.g., Galileo, Darwin") to support the existence of immaculate perceptions. It is *precisely* recent history of science that has driven many historians and philosophers to reject the thesis of immaculate perception (Brush 1974/1976), and the cases cited by Kendler—the scientific and Darwinian revolutions—are the cases most usually cited against his view. Scholarship in these areas is still unsettled, but it is at least clear that the old scientific heroes did not operate in the ways we used to think, and that history no longer supports the positivist, Whig conception of science (Brush, 1974/1976; Kearney, 1971).

Disagreements

Kendler defends the doctrine of immaculate perception because he thinks that rejection of it is "fundamental" to my rejection of operationism. This quarrel takes us into the more serious disagreements lying between myself and Kendler, and they deserve the fuller treatment I will give them in my fuller reply. For the moment I will simply observe that there are philosophers such as Hempel (one of the early Logical Positivists) and Suppe (1973) who do not accept Kuhn's (1970) assertion that all perception is theory-laden, but who nevertheless reject operationism (Hempel, 1954/1965; Suppe 1972). Later, I will show that it is possible to accept the existence of immaculate perceptions, while still rejecting the distinction between theoretical and observational terms that operational definitions were invented to bridge. In my original paper I did raise the "conceptual spectacles" argument as a possible objection to operationism, but we will find that it is not a *fundamental* objection.

Kendler proposes two examples of the utility of operational definition for actual psychological research, namely operational analyses of "intelligence" and "self-actualization." But what his analyses in fact reveal is the poverty of operationism. In the former case he stays close to operationism and ends up in a blind alley; in the second case his critique is penetrating and revealing, but departs from operationism.

With regard to "intelligence," Kendler argues that various operational definitions offered by Cattell, Binet, and Burt are "not commensurate." But rather than demonstrating the value of operational definition, Kendler's argument demonstrates one of its well-known failings: that it proliferates concepts which we believe ought to be the same but which by operational definition are rendered completely distinct. I raised this issue in my original paper (Leahey, 1980, p. 130), and Kendler has proved it. Cattell, Binet and Burt *claim* to be working on the same problem, intelligence, and each is free under operationism to define it as he chooses without challenge. Yet we are left with three research programs that can-

not be sensibly related. To blandly declare them "incommensurate" is remarkably Kuhnian and unhelpful. We find ourselves at a dead end in our researches on intelligence, and operationism offers no way out, only the possibility, even the probability, of a new definition and a whole new research program incommensurable with the historical predecessors on which science is supposed to build. Here, operationism's demand for operationally applicable definitions has distracted us from the hard theoretical thinking that might suggest what intelligence *is*, not just what it might arbitrarily be defined to be.

When Kendler points out that Maslow's treatment of "self-actualization" confuses facts and values in a seductive and misleading way, I think his point is well taken. But the excellence and acuteness of Kendler's treatment has nothing to do with operationism. Kendler shows that Maslow's trait-value connections are tautological; but operational definitions, *as definitions*, are *necessarily* tautological, just as "a bachelor is an unmarried male" is tautological. Maslow's definition can therefore not be rejected on operational grounds. Kendler argues that other psychologists with different values could produce different operational definitions of self-actualization, and that this fact destroys Maslow's account. But this would only proliferate concepts in the same way as with "intelligence," and Kendler did not conclude that anyone's account of intelligence was thereby rendered wrong, only incommensurate with other accounts. On Kendler's showing on operationist grounds, Maslow's theory of self-actualization is no more to be rejected than Cattell's or Binet's or Burt's theories of intelligence. What makes Kendler's critique effective is not operational analysis, but a persuasive argument that Maslow attempted to bridge the unbridgeable gap between facts and values.

A final disagreement concerns Kendler's parting, and hopefully Parthian, shot that anti-operationists are afraid of theory change and disproof of fond ideas. But I wrote that theories should "help us understand, explain and predict our experience" and that in the competition for scientific acceptance "inadequate theories will be discarded" (p. 140). I simply maintain that operationism is not required to bring about such desirable ends. All that is needed is a commitment to these values; formal recipes are stultifying.

The Major Issue

The most serious problem with Kendler's defense of operationism is summarized in his quotation from Bergmann (1954): "The root of the trouble was that some psychologists in their enthusiasm mistook the operationist footnote for the whole philosophy of science."

Kendler supposes that psychologists detached operationism from its philosophical commitments and made the sole error of overinflating its importance. Kendler goes on to defend operationism independently of its philosophical commitments as a pragmatically useful tool for the working scientist.

But I will maintain that while it is true that psychologists only thought they were swallowing operationism, they were in fact swallowing all of positivism. Positivism is the metaphysics that says there is no metaphysics. Operationism pretends to be a neutral tool that presupposes no particular psychology.

There is evidence in Kendler's own paper and sources that belie such innocence of philosophical entanglement. The section that heads Kendler's practical defense of operationism is headed "Operationism and Empiricism," and he speaks of defending operationism "within the context of the activities of the empiricists and theorists," in each case writing as if empiricism were not a large and involved philosophical system.

The Bergmann (1954) paper that Kendler cites also contains the following passage:

The impact on psychology was tremendous. Applied to psychological concepts, operationism becomes logical behaviorism, that is, a behaviorism sobered and shorn of its metaphysics.

which repeats Stevens' (1939) linking of operationism and behaviorism.

Operationism is not a presuppositionless, neutral tool, because there are no such tools. Even a screwdriver implies and assumes certain kinds of gripping appendages and certain kinds of objects. Operationism brings with it a secret commitment to positivism and dictates psychological behaviorism. In the name of the supposedly neutral task of facilitating communication it excludes alternative accounts of psychological science.

To demonstrate this requires more time and space than I have available, and will be the major burden of my next paper.

References

- Bergmann, G. Sense and nonsense in operationism. *Scientific Monthly*, 1954, 79, 210-214.
- Brush, S. Fact and fantasy in the history of science (1974). In M. Marx & F. Goodson (Eds.), *Theories in contemporary psychology*, 2nd ed. New York: Macmillan, 1976.
- Ghiselin, M. *The triumph of the Darwinian method*. Berkeley and Los Angeles: University of California Press, 1969.
- Hempel, C. A logical appraisal of operationism (1950). In C. Hempel, *Aspects of scientific explanation*. New York: The Free Press, 1965.
- Hull, D. Scientific bandwagon or traveling medicine show? In M. Gregory, A. Silvers & D. Sutch (Eds.), *Sociobiology and human nature*. San Francisco: Jossey-Bass, 1978.
- Kearney, H. *Science and change 1500-1700*. New York: World University Library, 1971.
- Kendler, H. *Psychology: A science in conflict*. New York: Oxford University Press, 1981.
- Kuhn, T. *The structure of scientific revolutions*, 2nd ed. Chicago: University of Chicago

- Press, 1970.
- Leahey, T. The myth of operationalism. *Journal of Mind and Behavior*, 1980, 1, 127-143.
- Stevens, S. Psychology and the science of science. *Psychological Bulletin*, 1939, 36, 221-263.
- Suppe, F. Theories, their formulation and the operational imperative. *Synthese*, 1972, 25, 129-164.
- Suppe, F. Facts and empirical truth. *Canadian Journal of Philosophy*, 1973, 3, 197-212.
- Suppe, F. The search for philosophic understanding of scientific theories. In F. Suppe (Ed.), *The structure of scientific theories*. Urbana, IL: University of Illinois Press, 1974.